Why the programs for new axioms need to be questioned
Solomon Feferman

The point of departure for this discussion is a somewhat controversial paper that I published in the *American Mathematical Monthly* under the title "Does mathematics need new axioms?" [4]. The paper itself was based on a lecture that I gave in 1997 to a joint session of the American Mathematical Society and the Mathematical Association of America, and it was thus written for a general mathematical audience. Basically, it was intended as an assessment of Gödel's program for new axioms that he had advanced most prominently in his 1947 paper for the *Monthly*, entitled "What is Cantor's continuum problem?" [7]. My paper aimed to be an assessment of that program in the light of research in mathematical logic in the intervening years, beginning in the 1960s, but especially in more recent years.

In my presentation here I shall be following [4] in its main points, though enlarging on some of them. Some passages are even taken almost verbatim from that paper where convenient, though of course all expository background material that was necessary there for a general audience is omitted.[1] For a logical audience I have written before about various aspects of the questions dealt with here, most particularly in the article "Gödel's program for new axioms: Why, where, how and what?" [2] and prior to that in "Infinity in mathematics. Is Cantor necessary?"(reprinted as Chs. 2 and 12 in [3]).

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My paper [4] opened as follows:

> The question, "Does mathematics need new axioms?," is ambiguous in practically every respect.
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> - What do we mean by "mathematics"?
> - What do we mean by "need"?
> - What do we mean by "axioms"?
>
> You might even ask, What do we mean by "does"?

Amusingly, this was picked up for comment by *The New Yorker* in its issue of May 10, 1999, in one of its little end fillers (op.cit., p. 50), as follows:

> "New" apparently speaks for itself.

I had to admit, they had me there.

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[1]The parts taken from [4] are reprinted here with the kind permission of the *American Mathematical Monthly*.

Part of the multiple ambiguities that we see in the leading question here lies in the various points of view from which it might be considered. The crudest differences are between the point of view of the working mathematician not in logic related fields (under which are counted, roughly, 99% of all mathematicians), then that of the mathematical logician, and, finally, that of the philosopher of mathematics. Even within each of these perspectives there are obviously divergent positions. My own view is that the question is an essentially philosophical one: *Of course mathematics needs new axioms*–we know that from Gödel's incompleteness theorems–but then the questions must be: *Which ones?* and *Why those?*

Let's start by making some preliminary distinctions as to the meaning of 'axiom'. The *Oxford English Dictionary* defines 'axiom' as used in logic and mathematics by: "A self-evident proposition requiring no formal demonstration to prove its truth, but received and assented to as soon as mentioned." I think it's fair to say that something like this definition is the first thing we have in mind when we speak of axioms for mathematics: this is the *ideal* sense of the word. It's surprising how far the meaning of axiom has become stretched from the ideal sense in practice, both by mathematicians and logicians. Some have even taken it to mean *an arbitrary assumption* and so refuse to take seriously what status axioms are to hold.

When the working mathematician speaks of axioms, he or she usually means those for some particular part of mathematics such as groups, rings, vector spaces, topological spaces, Hilbert spaces, and so on. These kinds of axioms have nothing to do with self-evident propositions, nor are they arbitrary starting points. They are simply definitions of kinds of structures which have been recognized to recur in various mathematical situations. I take it that the value of these kinds of *structural axioms* for the organization of mathematical work is now indisputable.

In contrast to the working mathematician's structural axioms, when the logician speaks of axioms, he or she means, first of all, laws of valid reasoning that are supposed to apply to *all* parts of mathematics, and, secondly, axioms for such fundamental concepts as number, set and function that underlie *all* mathematical concepts; these are properly called *foundational axioms.*

The foundational axioms correspond to such basic parts of our subject that they hardly need any mention at all in daily practice, and many mathematicians can carry on without calling on them even once. Some mathematicians even question whether mathematics needs any axioms *at all* of this type: for them, so to speak, mathematics is as mathematics does. According to this view, mathematics is self-justifying, and any foundational issues are local and resolved according to mathematical need, rather than global and resolved according to possibly dubious logical or philosophical doctrines.

One reason the working mathematician can ignore the question of need of foundational axioms–and I think that we [members of the panel] are all agreed on this–is that the mathematics of the 99% group I indicated earlier can easily be formalized in ZFC and, in fact, in much weaker systems. Indeed, research in recent years in predicative mathematics and in the Reverse Mathematics program shows that the bulk of it can be formalized in subsystems of analysis hardly stronger than Π^1_1-CA,[2] and moreover the scientifically applicable

[2] For predicative mathematics, cf. [3], Chs.13 and 14; for Reverse Mathematics, cf. Simpson [19].

part can be formalized in systems conservative over PA and even much weaker systems.[3] So, foundationally, everyday mathematics rests in principle on unexceptionable grounds.

Before going on to the perspectives of the mathematical logician and the philosopher of mathematics on our leading question, let's return to Gödel's program for new axioms to settle undecided arithmetical and set-theoretical problems. Of course, the part of Gödel's program concerning arithmetical problems goes back to his fundamental incompleteness results, as first indicated in ftn. 48a of his famous 1931 paper [6]. It was there that Gödel asserted the true reason for incompleteness to be that "the formation of ever higher types can be continued into the transfinite"; he repeated this reason periodically since then, but did not formulate in print the exact nature of such further axioms. An explicit formulation of the program in pursuit primarily of settling CH only appeared in the 1947 article on Cantor's Continuum Problem (and its 1964 revision in the light of subsequent events). It was also there that Gödel made the distinction between new axioms based on *intrinsic* reasons and those based on *extrinsic* reasons. Concerning the former he pointed to axioms of Mahlo type, of which he said that "these axioms show clearly, not only that the axiomatic system of set theory as known today is incomplete, but also that it can be supplemented without arbitrariness by new axioms which are only the natural continuation of those set up so far." ([7], p. 520) Since Gödel thought CH is false and recognized that Mahlo-type axioms would be consistent with $V = L$, he proposed other reasons for choosing new axioms; hopefully, these would be "based on hitherto unknown principles...which a more profound understanding of the concepts underlying logic and mathematics would enable us to recognize as implied by these concepts", and if not that, then one should look for axioms which are "so abundant in their verifiable consequences...that quite irrespective of their intrinsic necessity they would have to be assumed in the same sense as any well-established physical theory." ([7], p. 521)

My co-panelists are better equipped than I to report on the subsequent progress on Gödel's program in the case of set theory.[4] Briefly, the research in this direction has concentrated primarily on higher axioms of infinity, also known as large cardinal axioms (LCAs). These are divided roughly between the so-called "small" large cardinals such as those in the Mahlo hierarchies of inaccessible cardinals, and the "large" large cardinals, a division that corresponds roughly to existence axioms accepted on intrinsic grounds (or consistent with $V = L$) and those accepted on extrinsic grounds. The division is not a sharp one but falls somewhere below the first measurable cardinal.[5] By all accounts from the specialists, the high point in the development of "large" large cardinal theory is the technically very impressive work extending "nice" properties of Borel and analytic sets, such as Lebesgue measurability, the Baire property, and the perfect subset property–via the determinateness of associated infinitary games–to arbitrary sets in the projective hierarchy, all under the assumption of the existence of infinitely many Woodin cardinals.[6]

But the striking thing, despite all such progress, is that–contrary to Gödel's hopes–the

[3] Cf. [3], Chs. 13 and 14 for the claim about PA. Feng Ye has shown in his dissertation [20] that substantial portions of scientifically applicable functional analysis can be carried out constructively in a conservative extension of PRA.

[4] That was indeed stressed in the presentations of Maddy and Steel.

[5] Cf. Kanamori [11], p. 471.

[6] This is due, cumulatively, to the work of many leading workers in higher set theory; cf. Martin and Steel [15] and Steel's presentation to this panel discussion, for the history of contributions.

Continuum Hypothesis is *still* completely undecided, in the sense that it is independent of all remotely plausible axioms of infinity, including all "large" large cardinal axioms which have been considered so far.[7] In fact, it is consistent with all those axioms–if they *are* consistent–that the cardinal number of the continuum is anything it "ought" to be, i.e. anything which is not excluded by König's Theorem.[8] That may lead one to raise doubts not only about Gödel's program but its very presumptions. Is the Continuum Hypothesis a definite problem as Gödel and many current set-theorists believe?

Here's a kind of test of one's views of that: as has been widely publicized, a Clay Mathematics Institute that has recently been established in Cambridge, Massachusetts is offering what it calls Millennium Prizes of $1,000,000 each for the solution of seven outstanding open mathematical problems, including P = NP, the Riemann Hypothesis, the Poincaré conjecture, and so on. But the Continuum Problem is not on that list. Why not? It's one of the few in Hilbert's list from one hundred years ago that's still open. Would you feel confident in going to the scientific board of that institute and arguing that the Continuum Problem has unaccountably been left off, and that its solution, too, should be worth a cool million?

My own view–as is widely known–is that the Continuum Hypothesis is what I have called an "inherently vague" statement, and that the continuum itself, or equivalently the power set of the natural numbers, is *not* a definite mathematical object. Rather, it's a conception we have of the totality of "arbitrary" subsets of the set of natural numbers, a conception that is clear enough for us to ascribe many evident properties to that supposed object (such as the impredicative comprehension axiom scheme) but which cannot be sharpened in any way to determine or fix that object itself. On my view, it follows that the conception of the whole of the cumulative hierarchy, i.e. the transfinitely cumulatively iterated power set operation, is even more so inherently vague, and that one cannot in general speak of what is a fact of the matter under that conception. For example, I deny that it is a fact of the matter whether all projective sets are Lebesgue measurable or have the Baire property, and so on.

What then–on this view–explains the common feeling that set theory is such a coherent and robust subject, that our ordinary set-theoretical intuitions are a reliable guide through it (as in any well accepted part of mathematics), and that thousands of interesting and *prima facie* important results about sets which we have no reason to doubt have already been established? Well, I think that only shows that in set theory as throughout mathematics, a little bit goes a long way–in other words, that only the crudest features of our conception of the cumulative hierarchy are needed to build a coherent and elaborate body of results. Moreover, one can expect to make steady progress in expanding this body of results, but even so there will always lie beyond this a permanently grey area in which such problems as that of the continuum fall.

While Gödel's program to find new axioms to settle the Continuum Hypothesis has not been–and will likely never be–realized, what about the origins of his program in the incom-

[7] Interestingly, the only detailed approach we know of to settle CH that Gödel himself tried–first negatively and then positively–was not via axioms for large cardinals but rather via proposed axioms on scales of functions between alephs of finite index. Whatever the merits of those axioms *qua* axioms, his attempted proofs (c. 1970) using them proved to be defective; cf. [8], 405–425.

[8] Cf. Martin [14]. The situation reported there in 1976 is unchanged to date.

pleteness results for consistent formal systems extending number theory? Throughout his life Gödel said we would need new, ever-stronger set-theoretical axioms to settle open arithmetical problems of even the simplest, purely universal form, problems that he frequently referred to as of "Goldbach type". But the incompleteness theorem by itself gives no evidence that any open arithmetical problems–or equivalently, finite combinatorial problems–of *mathematical interest* will require new such axioms.

We're all familiar with the fact that the Π^0_1 statement shown undecidable by the first incompleteness theorem for a given formal system S (containing arithmetic) is cooked up by a diagonal construction, while the consistency statement Con(S) shown independent by the second incompleteness theorem is of definite *metamathematical interest*, but not of mathematical interest in the usual sense. Also familiar is the work of Paris and Harrington proving the independence from PA of a special finite version of Ramsey's Theorem, and, beyond that, the work of Harvey Friedman proving the independence of a finite version of Kruskal's Theorem from a moderately impredicative system and of an Extended Kruskal Theorem from the system of Π^1_1-Comprehension.[9] Each of these is a Π^0_2 statement shown true by ordinary mathematical means (i.e., in a way understandable to mathematicians without invoking any mention of what axioms they depend on, or of any metamathematical notions) and is established to be independent of the respective S by showing that it implies (or is even equivalent to) the 1-consistency of S, 1-Con(S).[10]

For a number of years, Friedman has been trying to go much farther, by producing mathematically perspicuous finite combinatorial statements φ whose proof requires the existence of many Mahlo cardinals and even of stronger axioms of infinity (like those for the so-called subtle cardinals), and he has come up with various candidates for such φ.[11] From the point of view of metamathematics, this kind of result is of the same character as the earlier work just mentioned; that is, for certain very strong systems S of set theory, the φ produced is equivalent to (or is slightly stronger than) 1-Con(S). But the conclusion to be drawn is not nearly as clear as for the earlier work, since the *truth* of φ is now *not* a result of ordinary mathematical reasoning, but depends essentially on accepting 1-Con(S). In my view, it is begging the question to claim this shows we need axioms of large cardinals in order to demonstrate the truth of such φ, since this only shows that we "need" their 1-consistency. However plausible we might find the latter for one reason or another, it doesn't follow that we should accept *those axioms themselves* as first-class mathematical principles.

My point here is simply that there is a basic difference between accepting systems such as ZFC + LCA, where LCA is the applicable large cardinal axioms, and accepting 1-Con(ZFC + LCA). As to the question of the need of large cardinal assumptions to settle finite combinatorial problems of the sort produced by Friedman, there is thus, in my view, an equivocation between needing a given axiom and needing its 1-consistency; it is only the latter that is demonstrated by his work. But if one does not grant that there is a fact of the matter

[9] Cf. Paris and Harrington [16] and, for results related to Kruskal's Theorem, Simpson [19], p. 408.

[10] 1-Con(S) is the statement of ω-consistency of S restricted to Σ^0_1 sentences; in other words, it says that each such sentence provable in S is true.

[11] In [4] I referred to Friedman [5] for his then most recent work in that direction. More recently, Friedman has been promoting rather different statements derived from Boolean relation theory; cf. my comments on Friedman's Urbana presentation below.

whether statements LCA of various large cardinal axioms are true, is there a principled reason for accepting 1-Con(ZFC + LCA) without accepting ZFC + LCA itself? Of course, if one does think that there *is* a fact of the matter as to whether such statements LCA are true, then the equivocation is a non-issue. But then, what is it that leads one to recognize LCA rather than its negation to be true?

Returning to the question of mathematical interest, there is not a shred of evidence so far that we will need anything beyond ZFC–or even much weaker systems–to settle outstanding combinatorial problems of interest to the working mathematician, such as those on the Millennium Prize list, nor is there any evidence that the kind of *metamathematical* work we've seen from Paris-Harrington to Friedman will bear any relevance to the solutions of these problems, if they are ever solved at all.

Thus, as I said at the outset, I think we are left to regard the question: Does mathematics need new axioms?, as primarily a philosophical one. And if you agree with me on that, then we have the discouraging conclusion that we can expect as many answers to the question as there are varieties of the philosophy of mathematics; among those that have been seriously supported in one quarter or another, we have the platonic-realist, structuralist, naturalist, predicativist, constructivist, and formalist philosophies.[12] In other words, if the problem is indeed a philosophical one, we can hardly expect an answer that will command anywhere near general assent.

But as a mathematical logician, if not as a working mathematician or philosopher of mathematics, I can end with a bit more positive conclusion. Even if mathematics doesn't convincingly need new axioms, it may need for instrumental and heuristic reasons the *work* that has been done and continues to be done in higher set theory. For example, in my own subject–proof theory–analogues of large cardinal notions have proved to be very important in the construction of recursive ordinal notation systems for the "ordinal analysis" of various subsystems of analysis and admissible set theory.[13] So far, these just employ symbols that act in the notation systems like "small" large cardinals, and do not depend on the assumption that such cardinals actually exist. The widespread appearance of analogue large cardinal notions (and, more generally, large set notions) also in admissible set theory, constructive set theory, constructive type theory and my own systems of explicit mathematics[14] suggests that there should be a general theory of such notions which includes all these as special cases. So far, these analogues correspond mostly to "small" large cardinals. At any rate, without the considerable work in higher set theory that led to such notions, these other areas of mathematical logic might still be back where they were in the early 1960s. It remains to be seen whether the bulk of that work, which is on "large" large cardinals, can have similar applications, and if not–why not.

[12]Perhaps one should even add the philosophy that is implicit in the view that category theory provides the proper foundation of mathematics. What to call it?

[13]Cf., e.g., Pohlers [17] and Rathjen [18].

[14]For these, cf. Aczel and Richter [1], Griffor and Rathjen [9], and Jäger and Studer [10].

Responses

Response to Maddy.

Maddy argues from a position that she calls the *naturalistic* point of view as to the philosophy of mathematics.[15] According to this, mathematical practice, and set-theoretical practice in particular, is not in need of philosophical justification. "Justification ... comes from within ... in ...terms of what means are most effective for meeting the relevant mathematical ends. Philosophy follows afterwards, as an attempt to understand the practice, not to justify or to criticize it." From that point of view, the original panel question is "a bit off target". Rather, "it would be more appropriate to ask whether or not some particular axiom ...would or would not help this particular practice...meet one or more of its particular goals." The example given, from contemporary set theory, is the assumption of many Woodin cardinals.

The naturalistic point of view in philosophy, as usually described, is that the entities to be admitted are just those posited by and studied in the natural sciences, and that the methods of justification and explanation are somehow continuous with those of the natural sciences.[16] One of the foremost exponents of the naturalist position in this sense is Quine and, according to his view, only so much of mathematics is justified as is indispensable to scientific practice. Thus, Maddy's use of 'naturalism' to describe her point of view is strikingly contrary to that, since her aim, above all, is to account for and in some sense give approbation to that part of current set-theoretical practice which accepts various large cardinal axioms (that happen to be inconsistent with $V = L$, among other things). This she does by taking mathematics in general and set theory in particular as a "science" to be studied in its own right, independently of its relationships to the natural sciences.

While Maddy keeps invoking mathematical practice in general in the scope of her naturalism, she does not reflect on the many instances in its history in which the question of what entities are to be admitted to mathematics and what methods are legitimate had to be faced, leading to substantial revisions from what's OK to what's not OK and vice versa. In binding itself to mathematical practice, this kind of naturalism is in danger of being unduly transitory. Even if one takes the proposed naturalistic point of view and mathematical practice as exemplified in set theory for granted, there is a crucial question as to what determines the "mathematical ends" for which the "most effective" means are to be sought. And having chosen the ends, in what sense does effectiveness justify the means? Why is it to be presumed that the "good" properties of Borel and analytic sets should generalize to all projective sets, given that they don't hold for all sets?

Maddy says her naturalist needn't concern herself with "whether the CH has a determinate truth value in some Platonic world of sets" or "confront the question of whether or not it is 'inherently vague'." Why then is it assumed that there has to be a determinate answer to whether all projective sets have the perfect subset property or the property of Baire, etc.?

[15] Maddy has elaborated this position in [13]. That is incidentally a retreat from her attempt in [12] to formulate a compromise between Gödelian platonic realism and Quinean scientific realism (one form of naturalism) that would justify current higher set-theoretical practice.

[16] Cf. (R. Audi, ed.) *The Cambridge Dictionary of Philosophy*, 2nd edn. (1999), p. 596.

Is there something essentially different about the character of these set-theoretical problems that makes the latter determinate but not (necessarily) the former? At any rate, admitting the possibility of some kind of indeterminateness for CH seems to me to be a slippery slope for the naturalist.

As a final point, Maddy suggests that I'm in favor of limiting mathematics, though "the essence of pure mathematics is its freedom." Surely she does not think that *anything* goes in mathematics. Old-style infinitesimals, Dirac delta-functions, unrestricted comprehension? If not, what justifies what is to be admitted to mathematics? Once it is agreed that there has to be *some* sort of justification, intrinsic or extrinsic, then one is in the game of potentially limiting mathematics in some way or other. I don't hew to any sort of absolute principle in favor of limiting mathematics.

Response to Steel

In his discussion of my contention that the continuum problem is inherently vague, Steel says that "if the language of 3d order arithmetic [in which it is couched] permits vague or ambiguous sentences, then it is important to trim or sharpen it so as to eliminate these...it may be that, in the end, our solution to the Continuum Problem is best seen as resolving some ambiguity."

It is useful, in response, to elaborate my ideas about vagueness more generally. These can be illustrated, to begin with, in the context of very familiar, set-theoretically low down mathematics. The conception of the structure $\mathbf{N}$ of the natural numbers is not a vague one (at least in my view); statements about $\mathbf{N}$ have a definite truth value, and the axioms of PA are among those and (on reflection) are evident for it. By comparison, the notion of *feasible* (or *feasibly computable*) *natural number* is a vague one, and inherently so; there is no reasonable way to make it definite. Though we might well admit certain statements about feasible numbers as being evident, e.g. if n and m are feasible, so is $n + m$, we cannot speak of truth or falsity of statements about feasible numbers in general. Nevertheless, the notion of feasibly computable number is sufficiently suggestive to act as a heuristic for a reasonable mathematical theory. Similarly, the notion of *random number between 1 and 10* is vague, but the conception of it makes it evident that the *probability* of such a number being less than 6 is 1/2. It is from such vague beginnings that substantial, coherent, and even robust mathematical theories can be developed–without committing oneself to a notion of truth as to the notions involved.

In the case of set theory, it is at the next level (over $\mathbf{N}$) that issues of evidence, vagueness, and truth arise. Once the conception of the structure of *arbitrary sets of natural numbers* is presented to us and we reflect on it, the axioms of second-order arithmetic ("analysis") become evident for it. Nevertheless, in my view, the meaning of 'arbitrary subset of $\mathbf{N}$' is vague, and so I would strongly resist talking about truth or falsity of analytic statements. In opposition to my view it might be argued that the structure of the continuum, when conceived geometrically, is not vague, and hence that analytic statements have a definite truth value via the interpretation of analysis in the real numbers. Probably if a poll were taken, few mathematicians would agree with me that the notion of *arbitrary real number* is vague, and so I would not want to make an issue of it. But I believe I would garner substantially greater support of my consequent view that the notion of *arbitrary subset of* *the real numbers* (existing independently of any human definitions or constructions) is vague, since we no longer have the anchor of geometric intuition there. Moreover, I would argue that it is *inherently vague*, in the sense that there is no reasonable way the notion can be sharpened without violating what the notion is supposed to be about. For example, the assumption that all subsets of the reals are in L or even L($\mathbf{R}$) would be such a sharpening, since that violates the idea of "arbitrariness". In the other direction, it is hard to see how there could be any non-circular sharpening of the form that there are as many such sets as possible. In the other direction, it is hard to see how there could be any non-circular sharpening of the form that there as many such sets as possible. It is from such considerations that I have been led to the view that the statement CH is inherently vague and that it is meaningless to speak of its truth value; the fact that no remotely plausible axioms of higher set theory serve to settle CH only bolsters my conviction. From the quote above, Steel is apparently willing to countenance an ambiguity in the notions involved in CH. If, as he puts it, the best thing then to do would be to resolve the ambiguity, it would show CH to be vague but not inherently so; that is the nub of our disagreement.

Relatedly, Steel characterizes my views as being instrumentalistic, which he takes to be a "dodge", but he oversimplifies my position in that respect. One kind of instrumentalism that I have espoused, to the extent that I have done so in one place or another, is very much a Hilbertian one (in the relativized sense): given a system S that one understands and accepts, if another system T is reduced to S, *conservatively* in the language of S, then that justifies the use of T, even if one does not grant definite meaning to the language of T beyond that of S. As an example, the overwhelming part (if not all) of scientifically applicable mathematics can be formalized in certain higher order systems T which are conservative over PA, and in fact much of it is already conservative over PRA; that thereby justifies the use and applications of such T (cf. [3], Chs. 13, 14). Similar results hold for the bulk of everyday mathematics (whether pure or applied) conservatively (for certain analytic statements) over constructively justified systems (cf. [3], pp. 201 ff). This kind of instrumentalism is thus philosophically satisfactory.

I have also argued (e.g. in [3], p. 73), that one's picture of the cumulative hierarchy is clear enough as a whole to justify confidence in the use of ZFC (and like theories) for deriving number-theoretical results. This is a *pragmatic* instrumentalism which is *not* philosophically satisfactory since there is thus far no philosophically satisfactory justification for ZFC, at least none in my view. But the result of the case studies cited above shows that though this kind of instrumentalism admits much more in principle than the preceding, there is no real difference in practice (i.e., with respect to the mathematics of the "99% of all mathematicians").

Response to Friedman

The core of Friedman's presentation consists of two daring predictions about the effect of his new work on Boolean relation theory, which it is claimed will eventually force the mathematical community to accept fully (perhaps after a period of controversy) new large cardinal axioms. Which those are is not specified, and in particular it is not said whether these will just be "small" large cardinals (presumably palatable to mathematicians with a certain amount of encouragement) or also "large" large cardinals. It is also not predicted how long this will take. If he is right, time will tell. If not ...? The criteria a-g he proposes for the adoption of new axioms set a very high bar (Olympic sized), but in my view appropriately so.

Finally, Friedman addresses my point that there is an equivocation between needing a large cardinal axiom and needing the statement of its 1-consistency (over ZFC). He says that the choices are essentially equivalent for the purposes of proving Π^0_2 statements. Of course. He also says that it is more natural to develop such consequences of, say, Boolean relation theory under the assumption of the axiom rather than the statement of its 1-consistency. I also agree to that. But I do not agree with his conclusion that this will show we "need" large cardinal axioms. It is neither here nor there that he means by "φ needs large cardinals to prove" that "any reasonable formal system that proves φ must interpret large cardinals in the sense of Tarski." If φ is equivalent to the statement of 1-consistency of a large cardinal axiom LCA and PA $\subseteq$ T and T proves φ then of course LCA is interpreted in T by the formalized completeness theorem. But that doesn't show that we need either LCA or its consequence φ in the normal sense of the word.

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